

★ FORUM ★

50 Years Later: A Conversation about the Biological Study of Language with Noam Chomsky

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It is not an overstatement to consider Eric Lenneberg the founder of the field of biology of language and his *Biological Foundations of Language* one of the field's founding documents. Similarly, modern linguistics in the tradition of generative grammar was founded by Noam Chomsky in the 1950s with his *Syntactic Structures* as one of the founding documents of this field.

At first, the work of Chomsky and Lenneberg as well as their respective seminal books may seem only vaguely related—after all, *Biological Foundations of Language* surveyed the biological literature while *Syntactic Structures* provided a formal analysis of natural language syntax. However, nothing could be further from the truth: Lenneberg and Chomsky co-founded what today is known as biolinguistics during their time as graduate students at Harvard. Even a quick look at *Biological Foundations of Language* gives this away: Chomsky contributed an appendix on “The formal nature of language” to the book. A closer look reveals that Lenneberg himself heavily relied on formal analysis (of language) just like that provided by Chomsky in order to advance his argument (in this context, see Piattelli-Palmarini, this issue, Becker, this issue).



Figure 1: Noam Chomsky portrayed by Jean-Baptiste Labrune (Creative Commons BY-SA 4.0).

Consequently, talking to Noam Chomsky as a co-founder of the field, contemporary, and friend of Eric Lenneberg was the obvious thing to do. Luckily, Professor Chomsky took the time to answer some questions about the early days of the field, his work and relation with Lenneberg, and a number of other questions and scientific issues that (still) captivate us 50 years later.

In this document, all questions posed by the interviewer (Patrick C. Trettenbrein) are printed in *italics*, whereas Professor Chomsky's answers are printed in normal type. Please note that this interview also includes some questions originally raised by colleagues and friends, which the interviewer has tried to reproduce here as accurately as possible. References to books and articles mentioned have been added for the readers' convenience.



First of all, let me thank you for taking the time to answer my questions. As you know, the reason for this interview is the 50th anniversary of the publication of Eric Lenneberg's monumental Biological Foundations of Language (1967), now widely considered one of the founding documents of the field besides, for example, your Syntactic Structures (Chomsky 1957/2002). I would like to start out by asking you about your relationship with Eric Lenneberg in the early days of biolinguistics. I take it the two of you first met during your time at Harvard?

I arrived at Harvard in 1951. Eric did at about the same time. We met very soon and quickly became close friends, the families too.

How closely were you working with Eric Lenneberg in those early days (and also later)? When re-reading Biological Foundations of Language at the end of last year it struck me how surprisingly modern many of his points of view were despite limited data he could draw from. Similarly, I was struck by how closely many arguments he puts forward align with what I would assume to be your points of view. Consider, for example, some of the points discussed by Massimo Piattelli-Palmarini (this issue): Lenneberg's take on language development, his considerations on a possible genetics of language, or on a more conceptual level, the distinction between competence and performance; even his ideas about the evolution of language. How big would you consider his influence on your thinking about these issues (and beyond)?

We spent a great deal of time together in the early 1950s, along with our mutual friend Morris Halle, discussing common interests, work we were doing, problems we were thinking about, papers and books we were studying, and the state of the fields in the areas of our interest. And lots more. What drew us together in this connection was a shared scepticism about much of what was virtual orthodoxy at the time in Cambridge: radical behaviourism and behavioural science more generally in the forms it was taking, the reigning doctrines of structural linguistics, euphoria about the prospects for new technologies, and a prevailing attitude, which seemed to us misguided, that these alleged breakthroughs would soon revolutionize the study of human thought and behaviour, casting out traditional mentalist mysticism.

Eric was beginning his investigation of biology of language in those years, but the insights you describe here—and there are many—are mostly from work of his years later. By the mid-1950s Eric and I left Harvard. He went to medical school, I went to the Massachusetts Institute of Technology (MIT). We remained in contact but not with the intensity of the early '50s when we were grad students at Harvard, meeting regularly.

Despite the enormous overlap between your points of view, as far as I could find out, the two of you never ended up publishing a joint paper—though you of course wrote an appendix to Biological Foundations of Language. In many respects, your work and that of Eric Lenneberg seem complimentary insofar as you focused on many conceptual issues and adopted a more computational respectively theoretical approach whereas he delved deeply into biology. How come the two of you never ended up publishing a joint paper?

During the years when we were in close contact we were graduate students. For some years afterward, publication possibilities altogether were limited for the kind of work in which we were engaged, which was quite isolated. And by the time publishing opportunities opened up we were pursuing somewhat separate if related paths.

More generally, could you elaborate a little bit on the early days? What led you and Lenneberg to pursue these questions, that is, what led you to look at language from a biological point of view?

To the three of us—Eric, Morris, and me—it simply seemed obvious that the human language faculty is part of human biology, much like the visual and other faculties. If so, it only made sense to try to incorporate the study of language within a general biological framework, the path that Eric pursued in his own way with such intensity and success.

With regard to Eric Lenneberg the person, do you remember what fostered his specific interest in language or where it came from? And do you any idea what made him go to medical school after already having earned his PhD in linguistics and psychology?

When we met, in 1951, we each had a special interest in language and psychology. After Eric finished his studies in linguistics–psychology, given his particular concern with biological foundations, it was quite natural for him to move on to med school.

This may be a somewhat odd question, because you of course wrote a chapter on the formal nature of language that was included as an appendix in Biological Foundations of Language, meaning that you obviously endorsed the book and what Lenneberg had to say about language and biology. Still, I would like to know what your overall impression of the book was when it first came out, respectively when you first got to read the manuscript?

I thought it was an instant classic, basically founding a new discipline. It is surprising, in fact shocking, that it is not better known. Just to mention one example, there has been an enormous growth of interest in evolution of language from the 90s—many publications, conferences, societies – but it’s quite rare to find a reference to Eric’s discussion of the topic in his classic book, though it was pathbreaking at the time and remains today one of the most sophisticated and advanced contributions to the topic.

Okay. Now, looking back 50 years later, what would you consider to be the book’s biggest achievement? Why do you think it is still relevant, respectively, why should researchers and students today still bother reading it?

The book developed a sound, deeply informed, comprehensive, carefully executed biological basis for the study of the human language faculty, its use and its evolution, not only bringing together what was known about this topic but substantially extending it, including provocative speculations that were far from conventional at

the time but have since in many cases been shown to have been on the right track. And while a great deal has been learned since, this study remains an indispensable source for inquiry into the biology of language in all of its aspects.

Yes, it is very impressive how modern Lenneberg's view of language development and the biological theory of language he sketches at the end of the book in a summarising chapter already were, despite the fact that a lot of the important empirical work on acquisition from which we can draw today had not yet been carried out. While some modifications and updates may be required here and there his theory is still very accurate. In earlier work (Lenneberg 1964) he already indicated that his long-term goal was to do exactly this: come up with a biological theory of language.

Still, it is interesting to note that Biological Foundations of Language in many ways was kind of a late comer to the party: Your review of Verbal Behavior (Chomsky 1959) had already had a huge effect on linguistics and psychology and some reviewers of Lenneberg's book at the time noted that, in a way, it seems that he not only sought to sketch a biological theory of language but also wanted to

[...] provide a palpable biological plausibility for conclusions to which a number of uncomfortable Empiricists [...] [had] committed themselves on the basis of formal argument alone. (Bem & Bem 1968: 498–499)

Do you agree?

When we arrived at Harvard, Skinner's William James lectures of 1948, the essence of his book *Verbal Behavior*, were widely regarded as the definitive approach to the psychology of language, in part because of the adoption of the basic framework by the highly influential Harvard philosopher W.V.O. Quine. Our triumvirate was almost alone in regarding it as seriously misguided.

My review, to which you refer, was submitted for publication in 1957, when Eric was already in medical school, but we had discussed these matters extensively in earlier years. The review drew from our reading and discussion of ethological literature, which was remote from the reigning orthodoxy. The rigidity of the orthodoxy is illustrated by the fact that even Karl Lashley's important 1951 paper on serial order in behaviour, which pretty much undermined Skinnerian behaviourism, was apparently unknown in the Cambridge behavioural science community. I couldn't find a reference in the relevant literature when I brought it up in the review (and I learned about it from an art historian, Meyer Schapiro). But these were the kinds of topics we were discussing. Eric's exploration of biological foundations was already underway at the time, though fully developed and published only a few years later.

So, you started out in Harvard by reading the European ethological literature in the original German—because Lenneberg grew up in Germany. Of course, you pointed out that language is actually best studied within an ethological framework and there are some obvious points about language that can be made which are still not being universally acknowledged. For example, the very straightforward and obvious idea that your language is a property of you and, first and foremost, your brain is still—maybe somewhat implicitly—deemed controversial when there are cognitive scientists who say that languages evolve(d)

in order to fit human brains (e.g., Christiansen & Müller 2015), whatever that is supposed to mean. What is your take on the current state of the biolinguistic research program today?

True: “whatever that is supposed to mean.” And yes, in the early ’50s, Morris, Eric and I were reading ethological literature. They were both fluent in German and read originals. I kept mostly to the English language literature. This was foreign territory in the Cambridge intellectual community of which we were part, and to the extent that it was known at all, it was dismissed. The idea that there could be an instinctive element in human language, or cognitive capacities generally, which seemed to us virtual truism, was regarded by the most influential figures as virtual nonsense. So it was a private preoccupation. Almost. George Miller was receptive to what we were thinking about, along with several fellow students. But few others.

In the years since, and currently, biolinguistics has been flourishing, thanks to much deeper understanding of the nature of language, its acquisition and use, but also in part to new imaging technology that has in some measure overcome the ethical constraints on direct experimentation—and unlike the study of vision and other capacities, experiments with other animals tell us very little because of the species-specificity of the basic properties of language. There are no homologous systems to investigate.

Digressing a little bit for second, I’d like to ask a related question that concerns linguistics as a field more generally. Your own and thus, by extension, Eric Lenneberg’s ideas about the scientific study of the language faculty are sometimes still portrayed as “controversial” even today—though it is interesting to see that Lenneberg is hardly ever referenced by critics despite having advocated a very similar point of view. Why do you think that is? One would think that it would be more attractive to attack someone who is no longer around to defend their views?

I don’t think that’s the reason. You have to ask who the critics have been. In the past 60 years, I’ve discussed the kind of work we (and by now a great many others) have been doing with people and audiences in many different disciplines. It’s often been considered highly controversial (if not absurd) by philosophers, linguists, and a variety of social scientists, but not by physicists, mathematicians, biologists, including distinguished figures, among them Nobel laureates in evolutionary biology, with one of whom I co-taught graduate seminars in biology of language in the 1970s.

Among the critics, Eric’s work was barely known, if at all, and if it had been known would have been considered “real science,” not subject to this kind of critique. In the hard sciences, explanatory theories that are developed at what David Marr (1982/2010) called the computational/representational and algorithmic levels are considered quite natural, and analogues are familiar in the disciplines. These are matters that have been extensively discussed. To mention one example, Jerry Fodor has devoted much of his distinguished career to explaining the validity of such approaches and countering criticism of them.¹

¹ Interviewer’s note: This exchange took place before the passing of Jerry Fodor at the end of November 2017.

Continuing with this topic for just a little bit longer, it seems that there are two different ways of doing linguistics, on some level maybe analogous to the way in which biology was done when it was still called natural history as opposed to how biological research is carried out nowadays. Norbert Hornstein speaks of linguistics as opposed to linguistics. To what extent do you think the entire discipline is still caught up in the “cultural history” stage, cataloguing what is found in the world, analogous to what the predecessors of biologists did when biology was still natural history?

The answer becomes clear when one compares what was appearing in the journals in the '50s with what appears in the (many more) journals today. It illustrates dramatically a change from *linguistics* to *linguistics* in Hornstein's sense. Furthermore, the typological range of languages investigated has vastly extended along with the depth of the questions examined, most of them unimaginable not many years ago. In this crucial sense then, “cataloguing what is found in the world” has vastly increased both in scope and depth.

Coming back to my previous question about the relation of your work to that of Eric Lenneberg, I would like to dwell a little more on the relationship of linguistics and biology. While the biological approach to studying language took shape in the '50s and '60s of the past century it seemingly took a while for the label “biolinguistics” to catch on and it has been popularised only in the past decades, amongst other things with the establishment of Biolinguistics, the journal in which this interview will be published. Recently, some people have complained that the label biolinguistics is kind of a rebranding of generative grammar, whereas actual biolinguistics should be understood more exclusively as a label for all biological investigation of the language faculty (e.g., Martins & Boeckx 2016). What is your take on this?

I don't see much of an issue. Biological investigation of the language faculty is, by definition, an approach to investigation of the language faculty. Generative grammar is the study of core properties of the language faculty. Why should any issue arise?

I agree. As I understood it, the criticism is about theoretical linguists mostly carrying on with their business “as usual” while labelling their work as “biolinguistics,” despite not actively seeking integration with biology. Boeckx & Grohmann (2007), in the inaugural article of this journal, labelled this the two senses of biolinguistics: the “weak” and “strong” sense; the former being that linguists still seek to uncover the properties of grammar and the latter referring to work that requires the integration of linguistic insights with those from other disciplines. Would you agree that this distinction should be made?

Individuals can choose their own research interests and projects. Clearly, however, the domain of “strong” biolinguistics, as defined, can be pursued only to the extent that “linguistic insights”—that is “properties of grammar”—have been developed sufficiently to be combined and integrated. Same quite generally, whatever the specific domain of inquiry: vision (as David Marr famously discussed; see Marr 1982/2010), insect communication, any other. Again, I don't see any issues.

When re-reading many of Eric Lenneberg's publications as preparation for this project it seemed to me that he probably would have agreed with the rather critical assessment that a true integration of linguistics and biology is still missing and that generative grammar has kind of "hijacked" the label biolinguistics. Obviously, there was a clear gap between linguistics and biology in Lenneberg's days and there still is today. Was he generally optimistic about an eventual integration of linguistics and biology? And are you yourself still optimistic about that, respectively do you think we are now closer to a "real biolinguistics" than 50 years ago?

As noted, I don't understand the "hijacking" issue. Surely a lot has been learned about the biology of language—biolinguistics—in the past 50 years. I don't know what a "real biolinguistics" is any more than what a real biology of vision is. There is increasing understanding of the topics. What more can we expect?

I admit that "hijacking" may be a bit strong of a term. Still, I actually had a quote by Lenneberg in mind. He wrote that

nothing is gained by labeling the propensity for language as biological unless we can use this insight for new research directions—unless more specific correlates can be uncovered.

(Lenneberg 1964: 76)

I suppose you would say that generative grammar offers the "new research directions" and that's why the issue doesn't even arise?

When a language is understood as a biological object, as in generative grammar (an I-language in contemporary terminology), then certain questions arise directly:

- How is language acquired?
- What is its neural basis?
- How did it evolve?
- How is it used?

Such questions cannot be formulated in any clear form if language is regarded as some kind of community property—say, a "sort of contract" in a community (Sausure) or "the totality of utterances made in a speech community" (Bloomfield). Accordingly, though not entirely neglected, such questions could be pursued only in limited ways in terms of such conceptions.

Furthermore, as noted earlier, it is virtual truism that such questions can be pursued seriously only to the extent that the properties of these biological objects are understood. It is not controversial that these "new research directions" have been developed in highly productive ways within the general "biolinguistic framework" that generative grammar adopted from its origins.

Against this background, consider the following quote from a report by Eric Lenneberg written in 1972, rescued from oblivion by Michael Arbib (cf. Arbib, this issue), a few years after Biological Foundations of Language including your appendix had been published:

At present there is only one type of scientific theory of language structure available, generative grammar, and this was never intended to serve as a model for

*biological mechanisms. The intent was to create a formalism that would adequately describe the web of relationships that characterizes a natural language such as English. For a time it looked as if it might turn into an algorithm or language analysis, but this is yet an unrealized dream (except for highly restricted discourse). Generative grammar does help us here and there to define the meaning of complexity in the structure of sentences, but because of the way it has been set up, it will also classify some utterances as complex (i.e., products of a long and complex history of transformations) that are intuitively the simplest of all—e.g., “Water!” which would have to be accounted for as a derivative of “Give me water!” which, in turn, is derived from a more basic form, “*You give me water.” What is most urgently needed is a theory of sentence production and comprehension that has the formal precision of Chomsky’s approach but is explicitly intended to explicate the psychobiological underpinnings of language capabilities.*

(Lenneberg 1972: 635–636)

Do you think that developments in generative grammar in the past decades have brought it closer to the theory of sentence production and comprehension that Lenneberg had called for? In many ways work within the Minimalist Program (MP) seems even further removed from a theory of language production and processing, instead focusing on the formal nature of the involved machinery?

I don’t entirely understand the quote. A generative grammar *G* of language *L* seeks to determine the structure of the infinite class of expressions of *L* and their interpretation at the conceptual-intentional (semantic-pragmatic) and sensorimotor interface (the role of the former primary, so recent work suggests). It is a theory of competence. Theories of performance for *L* will of course access the stored competence characterized by *G*; and general theories of performance will, correspondingly, access general properties of competence grammars. It all falls within biology, and all has “psychobiological underpinnings”.

There has been a great deal of progress in the study of language processing, including extensive and productive work on minimalist parsers, much of it appearing in a forthcoming book on minimalist parsers edited by Robert Berwick (Berwick & Stabler, in press). The study of language production has also progressed, though with a huge gap that holds for all voluntary action. As described figuratively by two leading researchers of voluntary action, Emilio Bizzi and Robert Ajemian (Bizzi & Ajemian 2015), we are learning a good deal about the puppet and the strings, but the role of the puppeteer remains a mystery. In the case of language, it was essentially this mystery that inspired the awe and wonder of some of the great founders of modern science, including Galileo, Descartes and the logician-linguists of Port Royal.

Of course I agree that it all falls within biology, theories of performance as well as competence. In my understanding of the quote, Lenneberg was—in part—pointing to the discrepancy between linguistics and biology that we already briefly discussed. It seems that the study of competence has isolated and still isolates linguistics from psychology and neuroscience; maybe even preventing a closer integration of these respective fields, as in parts of psycholinguistics?

Chesi & Moro (2015) have recently argued that competence and performance are actually interdependent. I would argue that there are essentially three possible scenarios in which the relation of grammar (G) and a parser as a performance system (P) could work out: (i) G could be independent of P, (ii) G could be accessed by P online during processing, or (iii) it could turn out that G is only implemented in wetware insofar as the totality of P's mechanisms gives rise to a system behaving in a way that is captured by the description of G. What are your thoughts about this? And how would you describe the relation of linguistics to psychology and neuroscience?

I don't understand any of this. The study of competence can't be isolated from psychology because it is part of psychology—unless we (perversely) define “psychology” to exclude internally-stored knowledge of language, arithmetic, etc. Psycholinguistics, for the past 50 years, has been closely integrated with the study of linguistic competence. How could it be otherwise? Same with neurolinguistics. Linguistic competence is represented in the brain (not the foot, not in outer space) and the same is true of performances that access this stored knowledge of language.

Speaking personally, I've always regarded linguistics, at least the aspects that interest me, as part of psychology, hence ultimately biology. The relation of linguistics to psychology is similar to the relation of the theory of vision to psychology: part to whole. And insofar as we are concerned with what is happening in the brain, it's integrated with neuroscience. In brief, I don't see how any of these questions even arise except under delimitation of fields that seem quite arbitrary and have never made sense to me.

Of course I agree that linguistic competence is not represented in the foot. What I was trying to get at is that, for example, it could be the case that this internally stored knowledge is only accessed during processing in the sense that it is built into the way in which the performance system works. Does that make any sense or am I completely off the mark here?

The I-language—linguistic competence—is accessed in every use of language: in processing linguistic input, but also production (including internal construction of thought). If it is “built into the way in which the performance system works,” then it must be duplicated in each performance system, which does not seem a reasonable proposal. I don't see any way of reformulating this idea that does not reduce to the assumption that I-language is a central system accessed by performance systems, much as knowledge of arithmetic is accessed in calculating.

Okay. You have recently co-authored a paper on the neural basis of language processing with Angela Friederici (Friederici et al. 2017), so you must be somewhat optimistic that real progress can be made in this area. What is your advice for researchers trying to bring linguistic theory and psychology or neuroscience closer together?

Not quite. I contributed some introductory remarks to her very important book (Friederici 2017a). On the rest, I don't think the formulation of the questions is helpful, any more than the question of how to advise researchers trying to bring the theory of visual perception and psychology and neuroscience closer together.

It doesn't seem to me the right way to formulate the issues. There are different approaches to the study of language (vision, etc.), and intriguing problems where they intersect. The advice is to pursue them.

On a related note, one of the biggest successes of the MP has been to reduce the complexity of the postulated cognitive machinery, for example, by relying only on a single operation which we think is at the core of the language faculty, that is Merge. Now, you yourself have speculated that it is not at all clear whether Merge is specific to language, at least in phylogenetic terms. With regard to how an operation like Merge may be implemented in wetware we are still in a situation where we can at best make educated guesses (e.g., Friederici, this issue), as we lack a linking theory between the computational/algorithmic and the implementational level. It seems to me that a scenario where circuitry capable of recursive computation may have evolved for another purpose (e.g., motor planning or music) and later was exapted for language seems quite likely. What is your current take on these questions?

It is often claimed that recursive computation is involved in motor planning, but that seems to me a misunderstanding of both recursive computation and motor planning. Recursive computation holds for systems of digital infinity. Motor planning is not a system of digital infinity (though one can impose an arbitrary digital grid on continuous systems, leaving the issues where they were). And as one looks beyond this initial (and crucial) distinction, divergences proliferate.

As for music and language, it seems to me far more reasonable to suppose that music (to the very limited extent that it involves recursive computation) was exapted from language than the converse. Or, perhaps, as suggested by Jeffrey Watumull and Marc Hauser in recent work (Hauser & Watumull 2016), that recursive computation emerged and was applied in cognitive systems of digital infinity, language and arithmetic, maybe music.

What about labels, respectively Label? You recently argued that Simplest Merge in contrast to the original definition of Merge cannot yield labelled objects by conceptual necessity, and continued by speculating that labels may not exist as syntactic objects. Could you elaborate on this? And what about endocentricity, respectively headedness as a key property of human language, after all, at the heart of X-bar theory?

This takes us off into different and more technical directions, not appropriate in this context, I think.

Well, some colleagues have asked me to pose some questions to you. One of them wanted me to ask you why you are so convinced that sensorimotor systems only play an ancillary role in language, that is, especially when taking a processing perspective? On the computational level this view is, of course, very plausible; but when we look at implementation she is not so sure. Think embodied cognition. Also, she thinks that in the context of the MP these ancillary systems are thought to do a lot more work at the interfaces than previously assumed?

I've explained the reasons elsewhere, both conceptual and empirical (for review of some of them, see *What Kind of Creatures are We?*; Chomsky 2016). They seem

to me sound and compelling. In processing, sensorimotor systems play a central role, by definition. I don't understand the rest. "More work" than what? "Than previously assumed" when? Implementation of what? I don't see what else is at stake.

Okay. Next, another colleague wanted me to ask you about your take on the origin of lexical elements which, incidentally, do a lot of the work in current syntactic theory. Lenneberg already put forward an approach to lexical semantics that was non-referential and completely intensional, the importance of which you have repeatedly emphasised. In Why Only Us? (Berwick & Chomsky 2016), lexical elements were almost completely put aside. Interestingly, Lenneberg seems to have thought that the way in which humans categorise is not qualitatively different from other animals. Also, he considered "words" not as labels for concepts stored in memory but as labels for categorisation processes. What is your current outlook on these questions? Will the evolutionary origin of "words" remain mysterious or eventually turn out to be susceptible to study?

I think there is very strong evidence, which I've discussed elsewhere, that human concepts/lexical semantics are radically distinct from anything known elsewhere in the animal world. Their evolutionary origin is a mystery, which is why they were "put aside" in our discussion of the evolution of the faculty of language. For the moment, there are, to my knowledge, no useful ideas as to how to investigate this mystery.

Lastly, if I am not mistaken, you started out at MIT as part of a machine translation project that you ended up never really working on because you thought it quite pointless—is that correct? A colleague of mine is interested in translation and, while not at all related to the topic of this special issue, he wanted me to also ask you about how you think one might approach the study of translation within a framework such as that of generative grammar? Is there something like a "translation faculty?" Personally, I might add that this question probably touches upon how we conceive of and understand the mapping to the semantic-pragmatic interface and the degree of variation that this mapping permits as opposed to the mapping to sensorimotor systems?

I happened to be appointed in a machine translation research project, but never worked on the topic. My feeling from the start was that for practical purposes, brute force approaches would be the most feasible. While some day understanding of language might contribute materially to this project, that time was still remote. And research on machine translation did not seem to me the way to advance the project of understanding the nature of language. These expectations have been borne out, as far as I know. I don't know of any reason to suppose that there is a "translation faculty". The question of variation at the semantic-pragmatic/conceptual-intentional interface is an interesting one, at the border of research (my own guess is: not much). But understanding is far from contributing much to improving automatic translation.

Final question. While this interview was intended as a discussion of your scientific work, I would nevertheless like to also include this somewhat more personal question, if that is

appropriate?—Obviously, you have not slowed down a bit despite officially retiring from MIT years ago. And rumour has it that you will soon be moving to the University of Arizona? So, after all these years in science and an arguably heretofore unprecedented academic career, please tell us, what keeps you going?

Yes, moving to the University of Arizona. What keeps me going is the excitement of learning and discovery. The fields that have always interested me are, I think, opening up new vistas that offer much hope of deeper understanding of the nature of language and mind.

Professor Chomsky, thank you very much for answering my questions.

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